Response to Comment on "Letter to editor: 'Defining Benchmarks for Transthoracic Esophagectomy A Multicenter Analysis of Total Minimally Invasive Esophagectomy in Low-risk Patients"'

Gutschow, Christian A

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Comment on ''Defining Benchmarks for Transthoracic Esophagectomy: A Multicenter Analysis of Total Minimally Invasive Esophagectomy in Low-risk Patients''

To the Editor:

We read with great interest the article by Schmidt et al[1] and greatly appreciate their aim to define ''best possible'' results of minimally invasive esophagectomy (MIE). Whether the benchmark values actually represent high standards or ''best possible'' results can, however, be questioned.

First, the study included 13 high-volume centers with inclusion criteria of more than 20 esophagectomies per year and commitment to esophageal surgery as documented by recent publications. The actual caseload in 8 centers was, however, less than 50 during 5-year study period. As reported previously in number of studies, esophagectomy outcomes improve after reaching annual caseload of 20 and overall number over 119 performed surgeries.[2,3] Also, if the case load was divided between more than 1 surgeon, it is possible to question whether these centers should have been included in benchmark analysis. The possibility of incompleted learning curves is highlighted in Figure 2[1] revealing high variation in the rate of complications between centers. Overall complication rate vary between 33.3% and 75.5%, and the rate of serious complications (Clavien-Dindo $\geq$3a) between 0 and 34.7%.

Second, the suggested benchmark value for 90-day mortality, which is considered better outcome measure for major surgery than 30-day mortality,[4] is 4.6% being actually higher than 4.1% reported in a population-based study from Finland and Sweden.[5] It is reasonable to ask whether we should aim our ''best possible'' results higher than average population-based results of any country.

Third, the suggested benchmark value for anastomotic leaks was 20% or lesser. As the authors discussed, the possible explanations for these high numbers are the strict adoption of Esophagectomy Complications Consensus Group (ECCG) criteria,[6] early learning curves, and changes in the surgical approaches during the study period.[7] Previously, the pioneer of this technique, James Luketich, was able to reduce the leak rate to less than 5% after adoption of intrathoracic anastomosis, omental flap, and optimal width of gastric conduit.[8]

Considering the fact that benchmark values were obtained from low-risk patients the complication rates seem to be high compared to previous literature. For example, in our medium-volume hospital in Finland[9] with 82 performed MIEs during 5-year period by a surgeon with completed learning curve, ECCG criteria–based Clavien-Dindo $\geq$3a complication rate was 6.1% (benchmark value $\leq$30.8%), anastomosis leak rate 3.7% (benchmark $\leq$20%), and 90-day mortality 1.2% (benchmark $\leq$4.6%) with significantly older study population (68 vs 58 yr).[10] This highlights that benchmark levels for MIE can be exceeded. In future studies, the emphasis should be not only in performing centers, but also in high-volume surgeons with completed learning curves.

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Olli Helminen, MD, PhD
Johanna Mrena, MD, PhD
Eero Sihvo, MD, PhD
Department of Surgery, Central Finland Central Hospital, Jyväskylä, Finland
olli.helminen@ksshp.fi

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Response to Comment on ''Letter to editor: 'Defining Benchmarks for Transthoracic Esophagectomy: A Multicenter Analysis of Total Minimally Invasive Esophagectomy in Low-risk Patients'''

Reply:

With great appreciation, we read Dr. Helminen's and coworker's comments referring to our recent publication.[1] Primarily, their criticism relates to the reference points for benchmark values that seem high compared with other outcome research on minimally invasive esophagectomy (MIE).

We are grateful for Dr. Helminen's remarks because they reflect a common misinterpretation of the benchmarking concept. Benchmark thresholds in our study are defined as the 75th percentile of the median outcome parameters from each collaborating center. Apparently, we failed to make it clear enough that our benchmarks represent *upper limits* of ''best possible'' results. This means that results from other research should be *within* the thresholds of the benchmark (the 75th percentile of the medians) to indicate acceptable outcome quality. This practice is in line with other studies, and we have detailed the mathematical background in our paper.

Another point of critique relates to the effect of learning, which undoubtedly is a paramount factor in a complex procedure such as MIE. Although we only included high-volume centers with an annual caseload of at least 20 esophagectomies (minimally invasive and other procedures), we agree that learning curves for MIE may have played a significant role in our dataset—and consequently, we have discussed this in the paper. However, the goal of our work was to present a snapshot of the actual situation, and we made it clear that our data will need an update in the

near future. We do agree, however, that future studies should focus on individual surgeon volume. A lower volume operated by a single surgeon may represent greater experience than a higher volume operated by many.

Nevertheless, we would like to stress that the outcomes reported in our study compare favorably with most recent publications. In this context, it is important to note that the <5% leak rate in Luketich's series exclusively relates to leaks requiring surgical revision (ECCG type III/Clavien Dindo grade 3b or higher). As the majority of anastomotic fistulae are managed conservatively (CD grade 2–3a), a considerably higher total leak rate (all ECCG types) in this series must be assumed.

Concluding, we would like to thank again Dr. Helminen and colleagues for their stimulating comments. Also, they should be congratulated for the truly excellent morbidity data (Clavien Dindo grade ≥3a: 6.1%; anastomotic leakage rate: 3.7%) of their own series that are unparalleled in the current literature and well within the benchmark thresholds set by our publication.

(on behalf of the EsoBenchmark working group)

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Christian A. Gutschow, MD
Division Head Upper Gastrointestinal Surgery
Department of General and Transplantation Surgery University Hospital
Zurich, Switzerland.
christian.gutschow@usz.ch

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Comment on "Re: Laparoscopic Intragastric Resection: An Alternative Technique for Minimally Invasive Treatment of Gastric Submucosal Tumors"

To the Editor:

We read with interest the recent manuscript by Dr Boulanger-Gobeil et al[1] regarding intragastric laparoscopy as a minimally invasive approaches to resection of gastric submucosal tumors. We agree that intragastric laparoscopy is a useful approach, but we challenge the assertions that their description is novel and lacking long-term follow-up.

The technique described by Boulanger-Gobeil et al, namely the use of conventional laparoscopic ports to perform intragastric laparoscopic resection, has been published by other centers, even for gastrointestinal stromal tumor (GIST), as was cited in the manuscript. Our own center published a variation on this technique for the same indication in 2000, followed by a series of a accumulated cases over the next 3 years.[2,3] The series included 13 patients, with 14 lesions and a median follow-up of 16 months. Tumors resected using an intragastric technique in our series arose from several areas of the stomach, but 5 were adjacent to the gastro-esophageal junction. The mean tumor size in our series was similar (3.8 cm), and all had an R0 resection with no recurrence noted in the follow-up period.[2]

There are a few key technical difference from that described by Dr Boulanger-Gobeil et al and the technique that we have previous published. First is the reduced number of ports with our approach. Typically, even if intragastric laparoscopic visualization was utilized, only 3 ports are used, rather than the 6 depicted in the manuscript by Boulanger-Gobeil et al. In terms of cumulative incision length and degree of soft tissue trauma, the difference between 6 ports and an upper midline laparotomy may be overstated. Second, small caliber laparoscopic instruments (sometimes called needleoscopic) were routinely used at our center.[2] These instruments, available from various manufacturers, have 2.3 to 3.0-mm shafts, and contain a mechanism similar to a Veress needle to facilitate insertion. Use of these instruments limits soft-tissue trauma to both the abdominal and gastric wall, while also limiting the size of gastrotomies needing later closure. Third, we routinely use a saline lift technique using a mixture of saline and epinephrine (1:10,000) to provide a fluid cushion around the lesion. This may provide a margin of safety when using monopolar electrosurgical devices that is common in these cases. Lastly, our technique differed in terms of the characteristics of lesions amenable to intragastric laparoscopic resection. Dr Boulanger-Gobeil et al, and also the author of the included commentary recommend that tumors need to be completely endophytic to be amenable to intragastric resection.[1,4] In our initial series, we confirmed that tumors with an exophytic or transmural component can be readily removed, since continuous intragastric insufflation keeps the stomach fully distended and only occasionally is external venting from the abdomen required. Laparoscopic suturing is equally successful with no added difficulty in suturing a full thickness defect compared a defect of only the mucosa. Self-locking sutures were not commercially available when our technique was published, but we would support the use of this material in this setting, to technically facilitate suturing. Our technique of introducing the suture transorally with the endoscope outfitted with a long overtube eliminates the need for a 10-mm intragastric trocar, and is suggested modification of the technique by Dr Boulanger-Gobeil et al. We have made a modification to this technique since original publication: use of endoscopic snare is not used routinely for specimen retrieval, in favor of polymer bag retrieval device.

The authors additionally state that no long-term follow-up is available for laparoscopic intragastric resections. Our center additionally published a follow-up study of these patients with a median follow-up of 61 months.[5] During that period, complete radiographic and endoscopic follow-up was available for 14 of the patients (93.3%) and no recurrences were noted. This follow-up period in our intragastric resection series is longer than the widely cited manuscript establishing long-term safety and efficacy of laparoscopic resection of gastric GIST tumors.[6]

We congratulate the authors on their successful oncologic management of an admittedly unique subset of gastric submucosal tumors. Existing guidelines regarding gastric submucosal tumors agree that management of masses <2 cm is controversial.[7] When gastric submucosal tumors are proven to be GISTs, post-resection pathologic evaluation suggests that gastric GIST <2 cm have a very low malignant potential, but the rate is not 0, and depends on a post-resection assessment of the mitotic index.[7] For small masses with associated symptoms such as bleeding or dysphagia, resection is easily recommended. Endoscopic surveillance is an alternative to resection for such small masses; however, there are currently no widely accepted protocol for such surveillance.[7,8] We further welcome what is likely to be renewed discussion of the role of this technique as numerous reports have emerged of successful management of proximal submucosal tumors with endoscopic submucosal dissection (ESD) or submucosal tunneled endoscopic resection (STER).[9,10] Intragastric laparoscopy will continue to be a valuable technique for surgeons, even as the field moves toward increasingly endoscopic resection approaches to even submucosal tumors.